

possible to perpetuate it. The fern-leaved form is, it can hardly be doubted, a reversion to an ancestral type which has been perpetuated in other species, and this may also be the case with the ivy-leaved form, though this is more obscure.

The races of *Oenothera* which De Vries has raised are nothing more than what a horticulturist would expect; and it may be conceded that if such races could hold their own in nature, distinct species might originate in this way. But there is no evidence that they do; and the probability of their being able to do so is against them. *Oenotheras* are pretty prolific where they occur, and so far as my experience goes they keep true to type.

Cultural mutations seem, as a matter of fact, to have little, if any, capacity for holding their own in the struggle for existence. I cannot call to mind a single instance of one which has been successful, and even in cultivation there is some reason to think that they are short-lived; but this is a point on which we are in urgent need of carefully ascertained facts. One is told, for example, that new varieties of the potato mostly cease to give satisfactory results after a few years. This is, however, a case of purely vegetative reproduction, and similar statements are made about the sugar-cane, which it is now hoped to regenerate by seminal reproduction. I can remember when potato-fields were covered with flowers and subsequently with fruit. I suppose it was thought antagonistic to tuber-production, as it probably was, and sterile races were selected accordingly. Prof. Hildebrand came to this country to study the subject, and I was able to supply him with information which I had collected for another purpose.

There is practically nothing to add to what has been said on the subject by Asa Gray ("Darwiniana," pp. 338-347). It is notoriously difficult to get hold of old cultivated strains of garden plants, and change of fashion hardly seems sufficient to account for the difficulty. Gray points out "that with high feeding and artificial appliances comes vastly increased liability to disease, which may practically annihilate the race." This has all but happened to the hollyhock, and, left to itself, the Phylloxera would have exterminated the vine in Europe. The existence of a species in nature implies a complicated adjustment to the surroundings. It is not sufficient to launch upon them a new form; in order that it may hold its own, the adjustment must be provided as well. It is by no means always an easy thing to transfer a species from one part of the earth's surface to another. The seed of the Kerguelen cabbage brought back by the *Challenger* germinated freely at Kew, but not a single plant was raised from the seedlings, which all succumbed to a ubiquitous *Peronospora*.

De Vries has done good service in directing attention to the study of mutations, the nature and origin of which deserve the most attentive study. They graduate into monstrosities which are even more mysterious. It is worth while directing the attention of those who are interested in mutations to the material which exists in Japanese horticultural books. Japanese taste in such matters is widely different from European. In the case of the garden convolvulus (*Ipomoea*), which is pretty stable with us, the Japanese have figured an extraordinary range of variations which no one else would dream of preserving.

W. T. THISELTON-DYER.

Witcombe, November 9.

The Winding of Rivers in Plains.

BEFORE writing to NATURE on the theory of winding rivers, it would have been wiser for me to have had some observations made as to the conditions of actual flow in the field in different circumstances. It is possible that the more complicated conditions which obtain in some places render the simple theory only partially applicable. My letter was immediately applicable rather to the flow in Prof. James Thomson's simplified model, where the artificial stream had a wooden bed, and the tendency to silt was indicated by short pieces of cotton pinned by one end to the bottom. It may be that the deposit of drift

on the inner side of some streams retards their flow by an unexpected amount; and probably there are other causes which prevent the James Thomson theory from being the last word on the subject. I do not pretend to be a field naturalist in any sense, and my cautionary note concerning the flow of glaciers I would ask readers to apply to the flow of rivers also, and to interpret the whole of my letter as a hint and exposition of theory rather than as an assertion and statement of fact.

OLIVER LODGE.

November 20.

SIR OLIVER LODGE's letter in NATURE of November 7 on the winding of rivers in plains has induced me to measure the velocity of flow in different parts of a bend in the river Wey near here. A short line—17 feet—was measured on the bank at the bend, and marks set up at right angles to it, and the time taken by blocks of wood to move between the marks measured with a stop-watch. The distances of the blocks from the inner bank were estimated in terms of the breadth of the river, with the following results:—

Distance from inner bank				Velocity in feet per second
0.3	0.30
0.5	0.45
0.6	0.55
0.8	0.71

This does not bear out his statement that "the flow is most rapid on the inner or sediment-depositing side of the bend," and that the water near the concave bank is nearly stationary, but upholds the common opinion of boating men and others.

The numbers refer to the surface flow only, and it is quite probable that there may be the undercurrent across the bed of the river; in fact, the sudden shelving so often noticed in rivers, and harbour channels where there is a strong tide, has led me to suspect for a long time such cross-currents.

The surface flow-lines are neither parallel nor straight. For this reason a short base line was used, and the velocities obtained are only approximate, but are certainly not far from the above values.

At the end of the experiments two blocks of wood were simultaneously floated down the stream, one near the inner, the other near the outer bank, and the latter won the race by twelve seconds.

However, I noticed that close to the outer bank (within 2 or 3 feet of it) there were back eddies forming a set of feeble whirlpools, and these may play an important part in the scouring.

R. C. SLATER.

Charterhouse, Godalming, November 17.

The Occurrence of Copper and Lithium in Radium-bearing Minerals.

It is possible that the remarkable action of radium emanation on copper, as recently announced by Sir William Ramsay (NATURE, July 18, vol. lxxvi., p. 269), may not be confined to solutions, but may also occur in the solid state. If so, it should be found that those minerals which contain both radium and copper contain lithium also.

In connection with another investigation, I had separated a sample of pitchblende, from Gilpin County, Colorado, into its principal constituents. The amount of copper in the sample was considerable. The final filtrate, remaining after the separation of the various analytical groups, contained only ammonium and alkali salts. After the evaporation of a portion of this solution, representing about 3 grams of the mineral, and the volatilisation of the ammonium salts, a small residue was left which, when examined spectroscopically, gave a very bright lithium line. This result led me to examine four other samples of uranium-radium minerals. These samples com-

prised a second specimen of pitchblende from the same locality as the first; carnotite from Montrose County, Colorado; gummite from North Carolina; and pitchblende from Bohemia. All the minerals, with the exception of the gummite, contained both copper and lithium in easily recognisable amounts. The qualitative analysis of 1 gram of the gummite showed no copper, but did show the presence of lithium in small amount.

The discovery of lithium and copper in uranium-radium minerals does not necessarily indicate the change of copper into lithium, since the presence of lithium may have been fortuitous; but assuming the accuracy of Prof. Ramsay's observation, the presence of lithium in uranium-radium-copper minerals is precisely what one should expect. The presence of lithium and absence of copper in the sample of gummite may be explained by the assumption that the change of copper into lithium has been completed. It may be added that even if further investigation should reveal the absence of lithium in any uranium-radium-copper mineral, the result would not constitute a valid argument against Prof. Ramsay's hypothesis, since the latter referred to copper in solution and not in the solid state.

HERBERT N. MCCOY.

University of Chicago, November 6.

A Convenient Formula in Thermodynamics.

It is possible that many teachers of thermodynamics may not have noticed that the characteristic equation for 1 kilogram of air takes the easily rememberable form $p v = T/10$, when p is measured in standard atmospheres, v in cubic feet, and T in thermodynamic centigrade degrees, the accuracy of the even integer being fully as great as that of the gas law itself. These units are, of course, a curious mixture of the English and Continental systems, but this seldom makes much difference in actual problems, and the convenience of the formula for rough mental computations is sometimes very great.

The data upon which this computation of the gas constant is based are the statements in the third (1905) edition of Landolt and Boernstein, that 1 litre of air under standard conditions weighs 1.2928 grams, and that an English yard is 0.91438 metre, and the value $T_0 = 273^{\circ}.13$ given by Buckingham in the Bulletin of the Bureau of Standards for May. The value $R = 0.1$ is consistent with these assumptions within less than one-fiftieth of 1 per cent.

The corresponding values of C_p and C_v , reduced from the mean of the results of Regnault (1862), Wiedemann (1876), and Witkowski (1896), are $C_p = 0.3467$ and $C_v = 0.2467$ cubic-foot-atmospheres.

Cambridge, Mass.

HARVEY N. DAVIS.

A Miocene Wasp.

IN NATURE of June 13, 1901 (vol. lxiv., p. 158), I described a curious variation in a bee (*Epeolus*), the second transverso-cubital nervure of the wings having its lower half absent. This aberration was evidently an example of "discontinuous variation," and from its occurrence in several specimens captured at the same place, it seemed that it must be inherited. There is a genus of Scoliid wasps, *Paratiphia*, in which the absence of the lower part of the first transverso-cubital nervure is normal. The species, found principally in the southern and western parts of North America, are quite numerous; and the broken nervure, looking exactly like the aberration described in the bee, is a good generic character. Nothing has hitherto been recorded concerning the past history of this genus, but I have before me a well-preserved *Paratiphia* from the Miocene shales of Florissant, Colorado, collected by Mr. S. A. Rohwer at station 14 in 1907. This insect, which I shall call *Paratiphia prae fracta*, is black, with the thorax large and robust (about 4 mm. long and $3\frac{1}{4}$ mm. broad); the head rather small (slightly more than 2 mm. diameter); the antennæ thickened; the abdomen constricted between the first and second segments, and parallel sided beyond; the hind

tibiae dentate on the outer side; the wings clear hyaline, anterior wing about 7 mm. long, with the large stigma very dark, the nervures light ferruginous. The specimen is a male. The venation is perfectly normal for *Paratiphia* in every respect, including the broken transverso-cubital vein.

It is certainly an interesting fact that a character like that of an imperfect vein, which can arise suddenly as a mere aberration, should persist from Miocene times (at least) to the present, and characterise a whole genus. From observations on bees and other Hymenoptera, it is evident that this modification has occurred many thousands of times without becoming permanent; that it has become so in the case of *Paratiphia* is therefore all the more remarkable.

T. D. A. COCKERELL.

University of Colorado, Boulder, Colorado,
November 7.

The Eggs of the Platypus.

SINCE writing the notice of Mr. le Souef's book on Australian wild life in NATURE for October 24 (vol. lxxvi., p. 635), I have been making inquiries as to the existence in collections of any examples of platypus egg definitely known to have been taken from the nest after extrusion. It has been suggested to me that Mr. Caldwell and Dr. Semon might possess such specimens. The former gentleman told me, however, some years ago that he never found an extruded specimen, and I learn from Dr. Semon that he was equally unsuccessful in this respect. In his letter he writes that "I have never found extruded eggs of *Ornithorhynchus*, but only intra-uterine specimens. To obtain the former, it would be necessary to open a very large number of burrows."

In the central hall of the British Museum is shown an egg-shell of a platypus sent from Queensland by Mr. G. P. Hill in 1902, but this, like Mr. le Souef's specimens, was doubtless found in its present broken condition.

Such broken shells might, apparently, be extruded from the uterus with the foetus; and, so far as I can find, there still appears to be no definite evidence that the eggs are really laid entire.

THE REVIEWER.

November 15.

Literature relating to Australian Aborigines.

IN NATURE of May 9 (vol. lxxvi., p. 32) I observed a communication from Mr. R. H. Mathews in which he makes certain statements imputing to me, by insinuation, what amounts to literary dishonesty. Will you kindly permit me to express my views on the subject?

Mr. Mathews says that I have "ignored" certain statements made by him in communications to scientific societies, and which were published before the appearance of my "Native Tribes of South-East Australia" in 1904, in which I record the same facts.

Mr. Mathews speaks of my account of the Dora ceremony, and makes the following insinuation:—"Dr. Howitt 'ignores' that I described that rite in January, 1900. If he did not avail himself of my work, which appeared four years earlier than his, then there is a wondrous agreement in our details."

My account of the Dora ceremony was given to me by Mr. Harry E. Aldridge in 1882. It was from his own experiences at the ceremonies on more than one occasion, and he had a knowledge of the tribal language.

Mr. Mathews also says that I "ignore" a map which he published in 1900, and which is substantially the same as one at p. 44 of my work. He adds the following sentence:—"In comparing the two maps and the explanatory letterpress accompanying mine, we observe a marvellous coincidence. Many other examples could be cited."

The map showing the native tribes of South Australia at p. 44 of my work was compiled from data supplied by the Rev. Otto Siebert, who obtained them by careful and protracted inquiries from persons knowing the several localities, as well as from personal knowledge. Practically